still occasionally found who, in defiance of all the rigidly scientific investigations of Brugmann, Osthoff, Henry, Sweet, Murray, and other philologists, persist, by disregarding phonetic and other ascertained linguistic principles, in connecting together utterly dissimilar tongues, such as the Indo-European languages, Hebrew, and Basque. The author of the above-named work is a writer of this type. His work bristles with philological impossibilities, and he appears to have no conception of the necessity of ascertaining, before comparison of one language with another, the laws which govern the sound changes of the languages compared and of the immediate groups to which they belong. The Hebrew word Satan he thinks is cognate with the Basque Tusuria "by transposition," and the work abounds in similar equations. The volume is unworthy of serious attention, and its only interest arises from its being one of those strange works that spring from the union of a certain kind of learned industry with misdirected ingenuity.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Fellowship of the Royal Society.

It is well known that under existing regulations the number of new fellows elected to the Royal Society every year is only fifteen. In this way the total number of fellows is kept at about 450. In the early days when this arrangement was made the limited annual number was doubtless sufficient to ensure the election of all the scientific men who really merited the honour, but since those days the scientific world has been growing larger and larger, and at the same time the general standard of work in all branches has become higher.

So long as the annual number of candidates was not more than forty or forty-five the selection of fifteen was not very difficult, and no man who had really done good work had to wait more than two or three years before election. Now, however, the annual number of candidates has increased to eighty or ninety, and this year it is said there were nearly 100 candidates.

Is it not high time, then, that the Royal Society took definite steps to make some change which would meet the requirements of the changed circumstances? Many of the older members of the society are well aware that the present state of affairs is unsatisfactory, and some have expressed their sentiments, but nothing has yet been done.

A simple plan would, of course, be to elect thirty new fellows every year instead of fifteen, but one can see objections to this plan. Has it ever been suggested that the Society should create an associateship and elect associates as well as fellows? The number of fellows might remain as it is, but if a limited body of associates was created, say fifty to begin with, and was increased by the election of twelve or fifteen every year, the pressure would be relieved, and I should think A.R.S. would be preferable to a long-deferred F.R.S. Subsequent elections of fellows could then be made from the associates, and this double election would give better assurance than now exists that none but the best men of the year were admitted to the fellowship.

ENQUIRER.

Earthquakes and Earthshakes.

Some of the memoirs, professedly seismological, which have appeared during the last year or two indicate that confusion has arisen from the use of the word earthquake in two distinct and independent senses. As this confusion seems likely to increase unless a modification of our nomenclature is adopted, the introduction of a new term appears to be requisite, however much this may be deprecated on other grounds.

In the generality of cases, the phenomenon represented by the word earthquake consists of a vibratory motion of the ground, of the nature of a wave motion, propagated outwards from a more or less extensive origin or focus. In some cases this disturbance may lead to damage or destruction of buildings, or even to displacement of the surface layers of the earth; but these are secondary results of the molecular displacements involved in the propagation of the wave motion, and, apart from them, the earth, after the earthquake has passed, resumes the same position and condition as before.

Occasionally, however, the word is applied to a disturbance of a wholly different kind, resulting in the formation of fractures and displacements of the solid rock, displacements which are molar and permanent, in the sense that the masses affected by them do not return to their original position after the earthquake has passed.

As the first was the sense in which the word is invariably used in Robert Mallet's classical researches, as it is that which has been sanctioned by long-continued usage, and as the proportion of records and observations, which do not apply to this phenomenon, is probably less than one in a thousand, I suggest that the word *earthquake* should continue to be used in this sense, and that for the other sense, in which it is sometimes used, the word *earthshake* should be substituted. Using the words in this way, we may say that earthquakes, or at any rate severe earthquakes, are frequently, if not invariably, caused by rupture of the earth's crust and the formation of fractures or faults in the solid rock, but these fractures, which are the primary cause of the earthquake, are only the secondary result of the earthshake, the action of which arises at a greater depth, and the ultimate cause of which lies beyond our present ken. The distinction is an important one, and the importance may be greater than will be acknowledged immediately, for some recent studies made by me have indicated a possibility that the earthshake has sometimes a greater extent than the earthquake; in other words, that the area over which permanent displacements of the earth's surface have taken place may be greater than the seismic area, or the area over which the shock was felt.

Incidentally, it may be mentioned that the whole of Prof. See's recent publications on the cause of earthquakes, and the greater part of those by Prof. Hobbs, deal with earthshakes and not with earthquakes as here defined. This is natural, for only the permanent changes, resulting from the earthshake, are of importance to the cosmogonist or the geologist; the transient displacements produced by the earthquake concern them, directly, but little, if at all.

R. D. OLDHAM.

Classification and Mathematics.

If mathematics is to be regarded as the science of classification, a view apparently taken in many recent works, it may be worth while to consider whether mathematical teaching should not begin with the use of models of classifications in general rather than with the special classifications in connection with which terms like straight line, rotation, product, power, &c., were originally introduced.

By a model of a classification is meant, for example, a set of things which can be classified by one respect as colour, and cross-classified by another as shape. Similarly, models can be made having three or four or more differentiations, in which any two differentiations have the relation of classification and cross-classification. If each differentiation is supposed to be ordered, we have then spaces of two, three, or four dimensions, of which the classified things form the points. By motion of a point in the space is meant its change in those properties which have been used in the classification. Consideration of the meaning of extension, rotation, and right angle shows the possibility of using the motion of extended bodies to construct a classification of the points of a space, even when we are unable to recognise the differentiations themselves of the space. This is the case met with in ordinary geometry.

As the foundation of geometry lies in the idea of ordered classification, so that of algebra lies in the conception of

correspondence between things. A function or one-to-one correspondence is a classification and cross-classification of the things which correspond. For example, a division of a number of models having different markings into two classes by colour and a cross-classification by shape gives a correspondence of the markings in one colour class to the markings in the other. If each marking in one class corresponds to the same marking in the other, we have the correspondence one. Similarly, various circular functions may be illustrated by models, beginning with transpositions. If things which correspond are called operands, and a correspondence of operands a function, then names seem to be needed to mean a correspondence of functions, and for the still higher correspondences which occur. In the usual school course we practically begin with the correspondences of functions, namely, of the numbers one, two, three, &c. It would seem more natural to begin with the correspondence, first, of operands to operands, and then of operands to functions, and define words as power, product, sum in reference to correspondences of operands illustrated by models. For example, a set of things the correspondence of which to another set is under discussion may be called a quantity. Two quantities which correspond to the same quantity correspond to each other; and their correspondence to each other is the product of the correspondence of one to the intermediate quantity and of the intermediate quantity to the other. In the case of vectors, since a vector is a correspondence of points, this would require the term product to be given to what is generally called the sum.

The properties of permutation, association, distribution should be considered in reference to tables of operands before considering tables of functions such as multiplication and addition tables. Space will not allow of discussing the illustration of addition, rule of signs, two-to-two correspondence, &c. The study of irrational numbers and continuous spaces should be postponed to a later stage.

Oundle. C. ELLIOTT.

An Emanation from Sodium.

DURING the course of some experiments upon the contact potential difference between the alkali metals and glass I noticed that a freshly cut piece of sodium rapidly discharged an electroscope.

Further examination showed that this action occurred only if the gold leaf was charged negatively. Little or no effect was produced if it was positively electrified. The action could be completely stopped by a membrane of celluloid sufficiently thin to give interference colours, and this fact alone points strongly to the discharging action being due to a vapour.

It was found, in fact, that a slight current of air directed so as to carry the supposed gas away from the charged plate of the electroscope enabled the leaf to retain its charge.

The effect is, however, unlike that met with in the case of phosphorus, since the vapour from that substance discharges both positive and negative electricity equally well. It does not, therefore, appear due to the air becoming ionised by a change occurring at the surface of the sodium, but more probably to the emission of an electrified gas. Experiment has shown that the rapid oxidation of the surface has little or nothing to do with the existence of the emanation, and it is very significant that all action ceases after prolonged heating (to melting point) of the metal. After some hours, however, the sodium shows signs of recovering its power to discharge a negatively electrified body.

Since all portions of the same block of sodium do not exhibit the action to the same extent, I am attempting to concentrate those parts which show it most strongly in order to determine whether some new radio-active body is present in the metal or whether there is a radio-active change occurring in the sodium itself.

A slight indication that the emanation is capable of depositing a radio-active layer of matter has been also noticed. The other alkali metals are now being examined and the whole matter fully investigated.

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WIND PRESSURE.

THE importance of a correct knowledge of the pressure exerted by the wind, as affecting the stability of modern structures, was brought prominently before the public by the disaster to the Tay Bridge on the night of December 28, 1879. At that time observatories at which wind pressure was directly measured were rare, the usual observed characteristic of the wind being its velocity as given by the Robinson cup anemometer.

At some stations both the Robinson cup anemometer and the Osler recording pressure plate were installed, and it was for this reason that in the report of the Royal Commission which was appointed in 1881 to consider the question, an attempt was made to state the relation between the probable maximum pressure which would be recorded in a gale and the maximum hourly run of the Robinson cups during that period. Also from records of pressure plates which were considered by the Commission to be not due to instrumental error depending upon momentum, but which represented real phenomena, it was decided that, for structures in exposed situations in this country, a maximum wind pressure of 56 lb. per square foot of surface should be allowed for in the design.

It was, however, felt by engineers at the time that this value, assumed uniform over the whole surface of a large structure, was very excessive, for, as the late Sir Benjamin Baker remarked at a discussion on wind pressure at the Institution of Civil Engineers soon after the report of the Commission was published, if such pressure actually obtained there ought not to be a bridge standing in the country. It was on this occasion that Sir Benjamin Baker stated his conclusions as to the nature of the motion of the wind and the pressures resulting from it, which theory was based, not on elaborate experiments, but on close observation of the behaviour of natural objects in the wind. In his words,

"If leaves and other light objects floating in an apparently steady current were watched it would be found that certain leaves would shoot forward at an increased velocity of 25 per cent. and upwards as compared with the mean velocity. Over a width of 20 feet at the centre of a wide and steady current the mean velocity might thus be constant, whilst over some particular width of 1 foot it might be momentarily fully 25 per cent. higher, and in the case of wind pressure 25 per cent. increase of velocity meant more than 50 per cent. increase of pressure. It was quite possible, therefore, that the large pressure boards might register a notably less pressure than the small boards, and might afford a clue to the reason why railway carriages were not upset when traversing lofty and exposed viaducts."

This appears to have been the first recognition of what may be called the variable structure of the wind as a factor of safety in the stability of structures, and it may be mentioned that the variation predicted by Sir Benjamin Baker was found to exist at points distant 11 feet apart in the experiments of Mr. Dines in 1894.

To test the truth of his conclusions Sir Benjamin Baker erected some wind-pressure plates on the site of the Forth Bridge, each provided with an arrangement for measuring the maximum pressures experienced. One of these gauges was 300 square feet in area, and the others $1\frac{1}{2}$ square feet. Taking the mean of the maximum daily readings for two years, the small-gauge indications were found to be 50 per cent. greater than the large-gauge indications, which was the result anticipated.

In experiments of this kind it is interesting to notice that there is one particular case in which with the